

April 16, 1950.

Mr. Ellis Englesberg,  
Department of Bacteriology,  
University of California,  
Berkeley 4, Calif.

Dear Englesberg:

Your news about the MRC and the Merck fellowships may have been discouraging, but probably not unexpected. The competition for these is fierce. Your best prospect is probably with the Public Health Service, but they are somewhat unpredictable, especially as they are now operating on a rather restricted budget.

I am afraid that we don't have any uncommitted funds for postdoctorate personnel in my laboratory. In general, the University encourages the use of its funds for the support of graduate students. However, if your fellowships are not awarded, I will certainly inquire as widely as I can. There might be some possibility of working out a collaborative project with some other people here who may have funds on hand, and who are interested in this line of work. Have you laid any alternative plans? If you could hold out for a year, some arrangement might be worked out here. But there is no point discussing this now.

Your account of the mutagenicity of formaldehyde for *Pseudomonas* seems quite convincing. The most troubling point is, of course, the dilution anomaly in counting the induced mutants. Your suggestion that you might be dealing with a non-specific mutagenic reactivation is not unreasonable. On the other hand, you might be running into modification of the phenomic barrier (Have you seen Davis' article in *Experientia*, 6:41-50, 1950 (Febr.) esp. pp48-51?). In either event, you might be able to get around the difficulty by using a suspension of untreated cells as a terminal diluent, subtracting their contribution to the mutant count. Killed cells might even work also.

In my own small experience with formaldehyde on *E. coli*, I found it very troublesome that there was a wide dispersion in the time required for colony formation by treated suspensions. The "viable counts" thus depended on the time at which the plates were counted. Have you run into this?

I haven't carried out any experiments on mutagenicity per se. Witkin mentioned that she had done an experiment or two with formaldehyde, with inconclusive results. I don't know the details. Whatever the outcome of the phage-resistance-mutation experiments, I would not abandon that line. Streptomycin resistance, although an infrequent mutation, is nearly as suitable as phage, and should also be tested.

We shall probably be arriving in Berkeley the last week in July. If you hear of any windfalls in respect to possible housing, I'd appreciate hearing about them, as we have made no plans yet.

Sincerely,

Joshua Lederberg